Review for paper "Momentum Flux characteristics of vertical propagating Gravity Waves" by P. Nyassor et al.

General Comments

The paper shows results from a case study of 2 mesospheric gravity wave events above São João do Cariri. They are able to demonstrate that the momentum flux differences between two different altitudes agrees with what is expected from theory with regards to upward and downward propagating waves. They show this in their figures and explanations in the text. Some refinement of the figures and explanations in the text are required, in my opinion, before it can be accepted for publication.

Specific Comments

This paper examines 2 nights of data and compares them. One of these nights the meteor radar data is not available, so they are not able to do a complete analysis comparison of the energy. Are these the only 2 nights with a similar period GW in all four filters over the 7 year dataset? Are there no others where the meteor radar is working so you can do a full comparison of momentum flux and energy?

Response:

So far, only two cases that have been with quite a good phase difference and similar periods were detected in four emission layers. However, we do have quite a number of cases of similar GWs detected in three emission layers. These cases were not selected due to the fact that we want to create a room for the detection of a fish bone structure (as it is in the case 04 – 05 December, 2004) in the phase propagation of the GWs.

With respect to the wind data from meteor radar, the data available was not processed and as at the time of submission of the manuscript. Also, since the initial objective was to investigate the dynamics of the momentum flux, we did not deemed it necessary whether we have wind data or not, hence, the omission of the estimation of the kinetic (total) energy.

Due to your comment and that of referee two, the wind data has been processed and the analysis has been performed on both momentum flux and energy for both cases.

Abstract – please mention that you are only looking at 2 events at the start of the abstract to aid clarity. You also mention reflected non-primary waves in the abstract, but surely this technique can just show that the wave observed is up/downward propagating, you can't say whether it is primary/non-primary or a reflected wave of any order?

Response:

Throughout – you use the term "energy" throughout the paper, but given the potential energy can only be examined for both events I would recommend altering this phrasing to reflect the results.

Response:

We appreciate your comment. In reference to your comment on the wind data for the case of 21 – 22 May, 2006, we have been able to obtain the data, thus the full analysis has now been realized, and hence a full but modified comparison has been done on the energy as well.

Section 2.1 – please include at least the altitudes of the different airglow layers at the start of this section to aid the reader or someone who is new to airglow studies. I know that you have pointed to the Nyassor et al paper, which does contain all the details, and mention them much later in the paper but the basics that are relevant to this study should be included early on.

This section has been rectified.

Section 2.2 – please include the height range that the radar observes at, you've mentioned the vertical resolution, but the height range is needed for context.

The altitude range of observation of the meteor radar has been included.

Line 124 – it is not clear what is meant by "19-25" hours. If the spikes in Fig. 2a are between 21-23 hours then the remaining dataset left is between 23 and 27, as per Fig. 2b-d, not 19-25 hours.

This has been corrected. In the new version, the time format has been changed. As a result, the sentence is "Due to the spike in the time series in Figure 3a, the data is limited to 23 UT on 04/05/2004 to 03 UT on 05/05/2004".

Section 3.3 – you mention Lomb-Scargle periodogram in this section but only show results from the wavelet analysis, is the L-S method used in this paper? If so, how closely does it match the wavelet results?

Thanks very much for the comments and questions. The periods present in the Lomb-Scargle are within the same range of the periods. Due to your question, Figure 5 and 6 had been included show the step by step analysis procedure and the Lomb-Scargle result.

Section 3.3 – the widths of some of the airglow layers overlap, does that influence your results at all? Also, the altitudes given in this section (see my earlier comment about section 2.2) are the average altitudes for the layers, but there has been work that shows that these layers do tend to vary in their altitude over time, would this affect your interpretation of the results?

Thanks for the question. The width of the emission layers do overlap but will not influence the interpretation of the results. The condition for a vertical propagating gravity wave is that the vertical wavelengths must be larger than the airglow layer thickness (typical full width at half maxima, ~8 km) can be observed at multiple airglow emissions almost simultaneously. Secondary, since bandpass filters are used to select the emission layer of interest, the variation of the emission layer attitude over time will not affect the interpretation of the results. The only possible effect will be either an increase or a decrease in the intensity of the layer.

Figure 3c – this is not very clear, the dotted lines all look near vertical apart from the last one in the bottom two lines. Maybe this needs to be highlighted in the figure. Also, it needs to be clear which of the two observed gw periods this is referring too or if they're combined somehow. E.g. Fig 5 is much clearer.

In Figure 3c, the reconstructed signal includes the two peak periods. The plot has been enhanced to depict the phase change using the dashed lines. Due to the size of Figure 3c (now Figure 7c), the Figure 5 (now Figure 8c) was produce in the results section in order to make ensure the phase difference are well depicted.

Figure 3d – is this just for the airglow intensity (photometer data) or the temperature data?

Yes, the wavelet was only applied to the intensities only. This is because the phase change is estimated using the intensities. For the temperature, they are used for the estimation of the momentum flux and the potential energy. From Figures 5 and 6, both intensities and temperature were subjected to the Lomb-Scargle. This was done to verify that the observed GW modulated the temperature profile.

Figure 4: - Figures e-h are duplicated in Fig. 7 – do they need to be?

This was done to put emphasis on the dynamics of the momentum flux, potential, kinetic and total energy. However, due to the comment of the referee 2, the Figures regarding this parameter has been modified.

- 4 g and h need to have the same X-scale as the rest of the plots (same for Fig. 7) to help with interpretation.

  Well-noted. Your comment has been implemented in a modified version.

- 4e and f – these are on a different temporal resolution than KE, could you show them on the same scale but with error bars on to represent the small-scale variability seen in the MF and PE plots.

Thanks very much for the comment. Due to the comment of the referee 2, the Figures and the respective analysis have been modified.

Section 6.2, 300-301 – can the phrases " a small fraction" and "a great amount" be replaced with something more precise please.

Rewrite this section when discussion.

Technical comments:

Line 124 – I think you mean Figure 2a not 1a

Figure 1a has been replaced with 3a.

Line 272 – the word wave is missing at the end of this sentence.

The word "gravity wave" has been included at the end of the sentence in Line 510 in the new version of the manuscript.

Line 273 – replace "using" with "calculated from".

The word "**using**" has been be changed to "**calculated from**"

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